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Coming to America: Carnap, Reichenbach and the Great Intellectual Migration

Part II: Hans Reichenbach

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Ich habe das Gefühl, dass gerade Amerika mit seinem Sinn für das konkrete und technische mehr Verständnis haben müsste für meine naturwissenschaftliche Philosophie als Europa, wo noch immer die mystisch-metaphysischen Spekulationen als die wahre Philosophie angesehen werden.

—Reichenbach to Sidney Hook, January 31, 1935

II.1. Introduction

In the late-1930s, Rudolf Carnap and Hans Reichenbach, arguably the two most prominent scientific philosophers of their time, emigrated to the United States, escaping the increasingly perilous situation on the continent. Once in the U.S., the two significantly changed the American philosophical landscape. In this two-part paper, I reconstruct Carnap's and Reichenbach's surprisingly numerous interactions with American scholars throughout the 1920s and 1930s in order to better explain the transformation of analytic philosophy in the years before and after the Second World War. In the first part of this paper, I reconstructed Carnap's contacts with American philosophers throughout the 1920s and 1930s. In this second part, I focus on Reichenbach's interactions with the American philosophical community before he moved to the United States. I argue that some of Reichenbach's work from the mid-1930s—in particular *Experience and Prediction* (1938)—can be better understood if we take into account the context in which it was written.

This paper is structured as follows. After an overview of Reichenbach's ignorance about Anglophone philosophy in the first stages of his academic career (§II.2), I reconstruct his 'American turn' in the early 1930s, focusing especially on the reception of his philosophy by a group of New York philosophers (§II.3). Next, I describe the increasing tensions between Reichenbach and the Vienna Circle (§II.4) and—after an intermezzo reconstructing Reichenbach's first attempts to find a position in the United States (§§II.5-II.6)—I outline his efforts to correct the American narrative about the role the Vienna Circle played in the development of scientific philosophy (§II.7, §II.9). In the final sections, I reconstruct how Reichenbach finally found a job with the help of Charles Morris, who, I show, played a crucial role in the development of scientific philosophy in the United States (§II.8, §II.10) and I

conclude this two-part paper with a discussion of the ways in which the story of Carnap's and Reichenbach's turn to America sheds new light on the development of logical empiricism.

II.2. Early insularity

In the first phase of his academic career, Reichenbach appears to have been largely ignorant about Anglophone developments in the philosophy of science. In debates about the philosophical implications of relativity theory—the focal point of his writings throughout the 1920s—Reichenbach almost exclusively concentrated on German interlocutors such as Moritz Schlick, Ernst Cassirer, and Hermann Weyl, who had all published influential analyses of Einstein's theory of relativity in the late 1910s and early 1920s.¹ Although there was a significant literature on relativity theory in the United States (e.g. Carmichael 1912; Tolman 1917) and in Great Britain (e.g. Robb 1914; Eddington 1918), he cited no contemporary Anglophone philosophers or scientists in *Relativitätstheorie und Erkenntnis apriori* (1920), the book in which Reichenbach first explicated his 'method of logical analysis', arguing that it is the task of philosophy to classify and order the fundamental assumptions of scientific theories. Reichenbach's ignorance about the Anglophone world is even more evident in his second book—*Axiomatik der relativistischen Raum-Zeit-Lehre* (1924)—which applies this method by developing an axiomatization of relativity theory. The book completely ignores the work of Alfred Arthur Robb, a Cambridge mathematician who had constructed a similar axiomatization about ten years before (Robb 1914). Whereas Reichenbach devised an axiom system which assumed "the relation of cause and effect" as a primitive term (1956, 25), Robb had developed a relativistic geometry on a purely causal basis, a view that has been dubbed the 'causal theory of space-time' (Winnie 1977). Unlike Weyl, who cited Robb's work in his influential Spanish lectures (1923, 8), Reichenbach seems to have been unaware that a comparable system had already been developed.²

Also in his early work on *probability*, the fundamental theme of his philosophy throughout his career, Reichenbach was surprisingly ignorant about Anglophone developments. Although John Maynard Keynes had made significant contributions to probability theory in the early 1920s, Reichenbach appears to have been unaware of Keynes' work until the latter's *Treatise on Probability* was translated into German.³ Nor did Reichenbach cite the work of Charles Sanders Peirce, who had already been developing a frequency theory of probability in the late 19th century. In fact, it was not until the mid-1930s, when he had just published his *Wahrscheinlichkeitslehre* (1935b), that Reichenbach discovered that his theory was "just in the line of [Peirce's] thought" (Reichenbach to Hook, October 19, 1935, HRP, 013-46-72).⁴

¹ See, e.g., Schlick (1917), Cassirer (1921), Weyl (1923), and, for a reconstruction, Ryckman (2005).

² See also Glymour and Eberhardt (2016): "Reichenbach's apparent unfamiliarity at the time with the English language literature on relativity is notable and unfortunate. He seems not to know of the work of ... Robb ... Whatever the cause, the *Axiomatization* ... would have been a different work, or none at all, had he taken account of those developments". Malament (2019, 336-8) argues that Carnap, too, was probably unfamiliar with Robb's work.

³ See, for example, Reichenbach's first reference to the translation (misspelling Keynes' name!) in "Kausalität und Warscheinlichkeit" (1930b).

⁴ In the translation of *Wahrscheinlichkeitslehre*, Reichenbach notes that he was not acquainted with Peirce's ideas when he wrote the German original (1949, 446n1).

Reichenbach's parochialism was not unique. At the time, many German philosophers almost exclusively focused on internal, Germanophone debates—a situation that often annoyed their American contemporaries. The New York philosopher Sidney Hook, for example, aptly described the problem in a report about his year in Europe:

What impresses the foreign student ... is ... [t]he insularity of German philosophy ... epitomized in the naïve declaration of one Privat-dozent that the history of philosophy ... since Kant is the history of German philosophy ... Whitehead, Dewey and Santayana, are hardly names ... William James seems to be the only American philosopher who is known—and he is more often 'refuted' than read. I have heard many refutations of pragmatism in Germany. Most of them were variations on the theme that man is born to something higher than merely to fill his stomach. (1930, 146)

Nor was this shortsightedness restricted to the *Schulphilosophen*, often accused of navel-gazing by scientific philosophers. When Ernest Nagel first visited a meeting of the Unity of Science movement in the mid-1930s, for example, he was surprised to learn that the attendants had no knowledge of Peirce's philosophy. In a report about the conference, Nagel wondered whether this testified "the provincialism of their reading habits" or the "inaccessibility of Peirce to European students" (Nagel 1934b, 592).

Accessibility issues definitely explain part of scientific philosophy's myopia throughout the 1920s. The story that Carnap had no access to the *Principia Mathematica* until Bertrand Russell provided him with a handwritten, 35-page summary of the book is well known.⁵ Something similar applied to Reichenbach. At the *Technische Hochschule* in Stuttgart, where he had been an instructor since the early 1920s, Reichenbach appears to have had very limited access to the foreign literature. When Reichenbach, in 1923, wrote his first letter to Russell, asking him whether he would be willing to join the editorial board of his proposed 'journal for exact philosophy',⁶ Reichenbach excused himself for his ignorance about Anglophone philosophy of science, explaining that he had had no access to the relevant literature:

Unfortunately, I have not been able to keep up with the foreign philosophical literature in recent years; we do not have access to that in my city. In consequence, it is impossible for me to determine how many scholars in England and America are working in our area ... I have not yet been able to obtain your main work *Principia Mathematica*, unfortunately. I believe that using your logical apparatus could also be helpful in dealing with problems in natural philosophy. (September 8, 1923, HRP, 046-03-13)

Reichenbach had learned about the *Principia* from Carnap, who had informed him about "Russell's symbolic system" shortly after he had received the latter's hand-written summary (Carnap to Reichenbach, December 26, 1922, HRP, 015-50-05). In general, Carnap seems to have been the driving force behind Reichenbach's growing awareness about developments in the Anglophone world. For when the former first visited the United States,⁷ he sent a long letter to Reichenbach in which he extensively reported on the rich English and American literature on relativity and probability:

⁵ See Carnap (1963, 14).

⁶ See section I.2.

⁷ Again, see section I.2.

People [here] are interested and excited about your *Axiomatik der RT*. Huntington told me immediately: we should have an axiomatization of relativity theory; and he was very pleased when I told him that one has already been developed. I also made use of the opportunity to inquire about the English and American literature in our fields.... Based on the books that people mentioned or showed me ... there is a lot of valuable and important work for us... For you, especially the following are worth considering: Whitehead, *The Principle of Relativity* ... Eddington, *The Mathematical Theory of Relativity* ... Keynes, *A Treatise on Probability* ... should be very good and important, provides an overview of the complete philosophy of probability. Carmichael's *The Theory of Relativity* ... contains a special section on the postulates of relativity theory, their independence, etc.; seems important. (Carnap to Reichenbach, May 7, 1923, HRP, 016-28-12)

Reichenbach, however, did not follow up on Carnap's suggestions. Except for his letters to Russell, his archives do not contain any correspondence with Anglophone philosophers of science in that period. In fact, even Reichenbach's *Philosophie der Raum-Zeit-Lehre* (1928), published almost five years later, still fails to discuss the Anglophone literature.

II.3. Berlin – New York

Reichenbach's access to American philosophy drastically improved in the late 1920s, when he took up a teaching position at Berlin's department of physics.⁸ Unlike the *Technische Hochschule* in Stuttgart, Reichenbach's new employer was a cosmopolitan institution with a world-leading physics department that attracted many international visitors and students. Not only did the move increase his access to foreign literature, archival evidence shows that Reichenbach, from the late 1920s onwards, started to be visited by a significant number of American scholars—including (but not limited to) the Berkeley physicist Victor Lenzen, the NYU philosopher Sidney Hook, the Columbia philosopher F. J. E. Woodbridge, and the Yale philosopher of science F. S. C. Northrop—who all attended his seminars and/or participated in meetings of his newly founded Berlin Group.⁹

Reichenbach's interactions with these American scholars had a tremendous impact on his subsequent career. The visitors confirmed Carnap's claim that the Americans were very interested in his exact approach to philosophy and Reichenbach started engaging in international debates about relativity theory—e.g. by explaining Einstein's theories in American newspapers and by starting a debate about Arthur Eddington's interpretation in the Anglophone journal *Philosophy*.¹⁰ Conversely, the American visitors helped spreading Reichenbach's work across the Atlantic. In the late 1920s, even before Blumberg and Feigl published their logical positivist manifesto,¹¹ Reichenbach's work started

⁸ The job had been arranged by Albert Einstein, Max Planck, and Max von Laue, who had created the special position (*nichtbeamteter ausserordentlicher Professor*) after the philosophers had refused to hire him because of his publications in a socialist newsletter in the 1910s (Hecht and Hoffmann 1982).

⁹ See Reichenbach's correspondence with Lenzen (HRP, 014-58), Northrop (014-57), and Woodbridge (015-51), as well as Hook (1978). For a history of the Berlin Group, see Rescher (2006) and Milkov and Peckhaus (2013).

¹⁰ See, for example, "Einstein's Theory Traced to Sources" (*New York Times*, January 26, 1929) and "Einstein's New Book Popular as Prize Novel" (unknown newspaper, January 30, 1929, HRP, 014-09-07) as well as Reichenbach (1931ab).

¹¹ See section I.3.

to gain some traction in the United States. His *Philosophie der Raum-Zeit-Lehre* was positively reviewed in American journals (e.g. Langer 1930; Northrop 1931) and his work started to be regularly cited in the English language literature (e.g. Wind 1927; Nagel 1929; Hook 1930; Knight 1929; 1930; Margenau 1931). Susanne Langer even characterized Reichenbach's approach as a new type of philosophy of science. Like Einstein, Weyl, and Whitehead before him, Langer explained, Reichenbach offered the "philosophical reflection of a scientist" rather than the "scientific speculation of a philosopher" (1930, 611). In general, reflections on the recent revolutions in physics were well-read in the U.S., which is perhaps the reason that Reichenbach was soon approached to have his popular book *Atom und Kosmos* (1930c) translated for the American market.¹²

Especially Sidney Hook's visit in the 1928-29 academic year appears to have boosted the American reception of Reichenbach's philosophy. For in the above-mentioned report about his year in Europe (see section II.2), the 29-year-old New York professor extensively discussed Reichenbach's philosophy, describing *Philosophie der Raum-Zeit-Lehre* as "the most lucid and comprehensive exposition of the philosophical implications of relativity that has yet appeared in Germany" (1930, 159). In addition, Hook provided an overview of the former's ideas about *probability*, showing that Reichenbach did more than 'just' work on the philosophical implications of relativity theory. In fact, Hook argued that Reichenbach's philosophy of probability was his "best work" and he described both the latter's frequency theory and his thoroughly anti-foundationalist approach to the philosophy of science, explaining that Reichenbach had developed a system in which all scientific knowledge is probabilistic in nature (ibid., 159-60).

Hook's report and correspondence from Germany appears to have significantly influenced his friends Ernest Nagel and Paul Weiss, who both, like Hook, had been students of Morris Cohen, the influential philosopher of science from the City College of New York.¹³ When Nagel started reading up on Reichenbach's work, he was impressed by the latter's "solidly based interpretation of science and its logic" (Nagel 1978, 42) and he had Hook sent a lengthy summary of his dissertation to Berlin (Hook to Reichenbach, November 3, 1930, HRP, 013-16-04). One year later, Nagel presented a paper on the frequency theory of probability at an APA conference, defending a variant that combined the advantages of Peirce's approach with those of his German contemporaries (Nagel mentioned Reichenbach, Richard von Mises, and Johannes von Kries), dubbing his view the "truth-frequency theory of probability" (1933, 544). In about the same period, Weiss learned that Reichenbach had started a new journal and submitted a paper. Reichenbach, who had been asking several American scholars to submit an article to *Erkenntnis* was delighted to receive Nagel's and Weiss' papers. After discussing the submissions with his student Carl Gustav Hempel, one of the newest members of his Berlin Group, Reichenbach decided to publish Nagel's and Weiss' contributions as the first American papers in *Erkenntnis*, adding short German summaries (written by Hempel) for the predominantly Germanophone subscribers (Nagel 1931; Weiss 1931). Thanks to Hook's report, in other words, Reichenbach and Carnap could now truly claim that *Erkenntnis* was an *international* journal for scientific philosophy.

In addition to Weiss and Nagel, Hook's report also appears to have influenced C. I. Lewis, who had recently published *Mind and the World-Order*, a book in which he, like Reichenbach, defends the view that "all empirical knowledge is probable only" (1929, 309). For a few weeks after Hook's report appeared in the *Journal of Philosophy*, Reichenbach also received a letter from Lewis (March 31, 1930,

¹² The book was translated by the mathematician Edward S. Allen from Iowa State University, who spent a year in Berlin in the 1931-32 academic year.

¹³ Nagel and Hook had been classmates, graduating in 1923; Weiss had received his degree in 1927.

HRP, 014-36), including a copy of the latter's book.¹⁴ Reichenbach read *Mind and the World-Order* in the spring semester of 1930 and appears to have immediately recognized that Lewis had developed a highly similar, anti-foundationalist philosophy of science. For in his response to Lewis, Reichenbach expressed his strong agreement with Lewis' view that "all judgments about nature have a probabilistic character" and asked him to submit a paper to *Erkenntnis* (July 29, 1930, HRP, 013-21-04). A few years later, these initial exchanges would ignite the seminal Lewis-Reichenbach debate about the presuppositions of probabilistic anti-foundationalism.¹⁵ Within only a few years' time, in other words, Reichenbach had developed a correspondence with a wide range of English and American scholars.

II.4. Berlin - Vienna

The growing American attention for his views on probability must have come as a welcome surprise to Reichenbach. For it was it was precisely this topic that had recently started to create a rift between Reichenbach and the Vienna Circle. Whereas Carnap, Schlick, and Reichenbach had formed a united front since the early 1920s, jointly pushing for the creation of a journal for scientific philosophy, they had started to develop into (philosophically) different directions in the late 1920s, when the Vienna Circle, influenced by Wittgenstein, had adopted a rather strict variant of truth-functional positivism. When Reichenbach presented his paper "Kausalität und Wahrscheinlichkeit" (1930b) at their joint conference on "the Epistemology of the Exact Sciences", members of the Vienna Circle strongly disagreed with his conclusions.¹⁶ Especially Schlick was strongly opposed to Reichenbach's view, as is evinced by their 1931 discussion about causality and probability in *Die Naturwissenschaften*.¹⁷ In fact, when Reichenbach started looking for a new job in the early 1930s, Schlick wrote a damning review to the Prussian Minister for Science, explaining that Reichenbach's work on probability was philosophically subpar:

[Reichenbach] was endowed with a rare talent analysing the basic concepts of natural science and he certainly made excellent use of this talent in his early writings that deal with the theory of relativity ... Reichenbach's later works do not stand on the same high level. I see his basic ideas on the analysis of causality and probability ... as failed. ... [A]s a researcher he [has] not been able to fulfil the hopes that had, justifiably been set in him ten years earlier. Whether personal traits are to blame for this or whether these very traits could lead him back to the path of truly productive research eludes my knowledge. (February 15, 1931, MSP, 112/Preu-6, translation from Stadler 2011, 42)

¹⁴ It is also possible that Lewis learned about Reichenbach's probabilism from Weiss, who had enrolled as a graduate student at Harvard in the late 1920s.

¹⁵ For a reconstruction of this debate, see Atkinson and Peijnenburg (2016).

¹⁶ See, for example, Carnap's contribution to the ensuing discussion: "Mr. Reichenbach said that probabilistic statements about the future can be neither validated nor refuted by ... future experiences ... In my opinion, the meaning of any statement is only determined by what it says about the possible contents of experience" (Carnap et al. 1930, 268). That he was seriously concerned about Reichenbach's position becomes clear from a note, written shortly after the conference, which ranks all metaphysicians "according to their degree of bad-ness". Reichenbach is included on the list as a "modest offender" (Oct. 21, 1929, in Carus 2007, 107).

¹⁷ See Schlick (1931), Reichenbach (1931c), and, for a reconstruction, Padovani (2010).

A second cause of the growing opposition between Berlin and Vienna were Reichenbach's ideas about the nature of scientific philosophy.¹⁸ In Reichenbach's view, it is the job of the scientific philosopher to aid scientists in making their theories more epistemologically coherent, as scientists are often too busy to think about the foundations of their theories. The Vienna Circle, on the other hand, believed that there is a strict distinction between science and philosophy; philosophers should stick to analyzing the *language* of science. In his opening statement for the first issue of *Erkenntnis*, Reichenbach aimed to describe the goals of the new journal by explaining the difference between traditional and scientific philosophy. The problem with traditional philosophy, Reichenbach argued, is that it has the "tendency to ... concentrate on what is not yet known". *Erkenntnis*, however, would be a journal for scientific philosophy—a philosophy of that what *is* known, i.e. the results of the special sciences (Reichenbach 1930a, 2-3). When Reichenbach first sent a draft of his statement to Schlick, the latter responded very negatively, suggesting that he contradicted some of the central claims of the Viennese manifesto (Hahn, Neurath, and Carnap 1929). Traditional philosophy is meaningless and to portray *Erkenntnis* as a journal for a scientific version of philosophy would be to deny their radical rejection of the tradition. In April 1930, Reichenbach received a letter from Carnap, explaining that Schlick would step down as an editor if he did not change his opening statement:

Yesterday I received your draft of the opening statement... I was able to discuss the document with [Schlick] straight away. We cannot sign this statement under any circumstances. And Schlick even said that if this is published as the programmatic paper ... he will not be able to stay on as an editor. You are making a concession to traditional philosophy here, which has very much surprised me... We, and also you, are in reality not of the opinion that the differences between our views and traditional philosophical systems are just based on what is not known yet. It should be said that most of those systems contain metaphysics, and that we believe that metaphysics is meaningless. And when you speak of philosophy as a science, it almost literally contradicts with what we said in ... "The Scientific Conception of the World". (April 29, 1930, HRP, 013-41-66)

Reichenbach, however, refused to make substantive changes, warning that the Wiener Kreis would be making the same mistake as the *Schulphilosophen* if they were to create a "specialized terminology" only comprehensible to a selected inner circle. Although he was interested in their view that philosophy cannot technically be a science, it would be a mistake to put this into the journal's mission statement (Reichenbach to Carnap, May 6, 1930, HRP, 013-41-65). Shortly thereafter, Reichenbach received a letter from Schlick, announcing his resignation and explaining that he had no interest in being an editor of a journal that made concessions to traditional philosophy (June 8, 1930, HRP, 013-30-28).

II.5. The Great Depression

In the early 1930s, shortly after Schlick's resignation, Reichenbach started to have serious financial problems. The Weimar Republic had been badly hit by the Great Depression and its economy had shrunk by 25% in the years after the Wall Street Crash of 1929. American banks, heavily strained by the crisis, collectively withdrew their loans to German companies and unemployment rates almost quadrupled between 1929 and 1932. Reichenbach still had his job at Berlin but it was so poorly paid

¹⁸ For a reconstruction, see Carus (2007, 209-18), Milkov (2013), and Dewulf (2020, §2)

that he had a hard time keeping his head above water.¹⁹ Even after his wife Elisabeth took up a teaching position to bring in extra money, Reichenbach still regularly had to beg a colleague from the dentistry department to help them with their bills (Reichenbach to Dieck, Jan. 12, 1933, HRP, 014-47).

Reichenbach's career prospects were not very promising either. Whereas many of his colleagues had obtained prominent chairs at prestigious German universities, Reichenbach knew that he had little chance of finding a professorship in the current intellectual climate. In the 1930s, German philosophy was dominated by idealism and phenomenology and there was little space for a philosopher whose work was so intertwined with the sciences. In a letter to Berliner, Reichenbach wrote:

You know that ... the situation in Germany is particularly difficult for me as German philosophy is completely dominated by a historical-humanistic approach [*historisch-geisteswissenschaftlich eingestellt*] and rejects the scientific attitude. In consequence, I have failed to obtain a regular professorship [*etatmäßige Professur*] in Germany up to now, even though my work has been widely recognized by natural scientists... Philosophical chairs are generally not awarded to natural philosophers here.²⁰ (April 20, 1932, HRP, 014-56-10)

Reichenbach particularly blamed Adolf Grimme, the German minister of culture, for the situation. In a letter to Sidney Hook, Reichenbach explained that Grimme was a former student of Husserl and that he was largely responsible for the unfavorable academic climate:²¹

In academic philosophy, phenomenologists have all the power because the socialist minister of culture is a former student of Husserl; he offered Heidegger a position at Berlin and, after he refused, Nicolai Hartmann; the faculty had suggested Cassirer, but the minister did not care. Oskar Becker, also a phenomenologist, has recently been offered a job at Bonn. We are therefore not in an easy position at the moment. (September 15, 1931, HRP, 014-51-25)

Especially the appointment of Becker at Bonn must have been disillusioning. Reichenbach himself had been considered for a chair at Bonn's *Physikalisches Institut* (Konen to Reichenbach, June 14, 1929, HRP, 014-09-08) but he had never received a formal offer.

Because of his discouraging career prospects, his financial situation, and the increasing interest for his philosophy across the Atlantic, Reichenbach started to consider emigrating to the United States. Inspired by Alfred Landé and Herbert Feigl, two former colleagues who had recently obtained positions at U.S. institutions, Reichenbach began to contact several of his American acquaintances (e.g. Sidney Hook, Edward Allen, and Frederick Woodbridge) to help him arrange a guest professorship.²² In the above-mentioned letter to Berliner, for example, Reichenbach continues:

¹⁹ Although Reichenbach's move to Berlin had been an improvement from an academic point of view, the position paid less than his job in Stuttgart. See Traiger (1984, 504).

²⁰ Reichenbach was certainly not the only one who believed that he should have obtained a chair by now. In a letter to Carnap, Bernhard Bavink argued that it was a "scandal" that someone of Reichenbach's stature still had not obtained a professorship (October 29, 1932, RCP, 028-04-04).

²¹ What Reichenbach did not know is that Schlick, in writing a negative assessment of his work, had not been helping him either (see section II.4).

²² See (HRP, 014-51-[20-28]) and (HRP, 013-39-15). Reichenbach likely chose the route via a guest professorship because both Landé and Feigl had been offered positions at American institutions after they had already spent a year in the United States. Landé had visited Ohio State University twice (1929,

There is a lively interest in our work abroad ... In America people have written about our work several times. Furthermore, I always have American professors and students among my listeners here and I know from them that there is interest in our exact approach to philosophy in America ... In these circumstances, I have asked myself whether it would not be possible for me to find a job in America. (April 20, 1932, HRP, 014-56-10)

Berliner, who was the editor of *Die Naturwissenschaften* at the time, promised to put in a good word for Reichenbach with Abraham Flexner, one of the best-positioned academics to bring German scholars to the United States. Flexner had recently founded Princeton's Institute of Advanced Study and was about to announce Einstein and Weyl as some of the Institute's first faculty members.²³ Flexner told Berliner that he was interested but that he would have virtually no chance of finding Reichenbach a position in the coming two years (June 30, 1932, HRP, 014-56-07). American academia, though more sympathetic to scientific philosophy, was as badly hit by the great depression as the Weimar Republic.²⁴

II.6. Paradise Lost

Reichenbach's search for a position in the United States was seriously interrupted in 1933. In March of that year, the Nazi party obtained almost a majority in what would turn out to be the last multi-party elections of the Weimar Republic and the Reichstag passed the infamous *Ermächtigungsgesetz*, thereby effectively turning Hitler's government into a full-blown dictatorship. On April 7, the new regime passed the notorious "Law for the Restoration of the Professional Civil Service", which soon led to the termination of hundreds of contracts at German universities. Especially the University of Berlin was greatly affected, terminating 278 (out of 797) contracts of mostly Jewish academics after the law took effect (Grüttner and Kinas 2007). In the same month, the German student union started to organize actions against the 'un-German spirit', which resulted in massive book burnings across the country, including much work that would later be labeled as "Jewish physics" (Lenard 1936). Reichenbach needed a new job immediately. Or, as he would express it to the American physicist Robert Millikan: "Germany is closed to a man who has two Jewish grandparents and called Einstein's theory the greatest discovery in modern physics" (October 18, 1933, HRP, 013-50-39).

In response to these developments, Reichenbach seems to have largely dropped his attempts to find a job in the United States. Any position outside Germany would do now and his experiences of the

1930-31) before he was offered a professorship and Feigl acquired a position at the University of Iowa after spending a year at Harvard (1930-31).

²³ Einstein was presented as one of the institute's first appointments on October 11, 1932. The announcement about Weyl followed a few months later (*New York Times*, January 10, 1933). It is not clear whether Reichenbach knew about the plans for the Institute when he asked Berliner to recommend him to Flexner. Still, Reichenbach refers to Flexner as the person who has "the biggest influence" in the "American academic world" (April 20, 1932, HRP, 014-56-10).

²⁴ Reichenbach had more bad luck. In 1931, Paul Weiss and Herbert Feigl tried to arrange a position for Reichenbach at Harvard, likely because they knew that especially Lewis was heavily interested in the latter's work on probability. Feigl tried to convince Lewis to offer Reichenbach a guest professorship while Weiss tried to work R. B. Perry (Feigl to Reichenbach, July 25, 1931, HRP, 013-11-06). It is unclear why their attempts failed but it is likely that the depression played a role here as well. In a different letter, Allen cites the Harvard mathematician George Birkhoff as explaining that his department preferred to use its scarce funds to help unemployed American scholars (HRP, 013-39-15).

last few years had likely convinced him that he would have a better shot of finding a position closer to home. There is no evidence that he asked any of his American correspondents for help in the spring semester of 1933, likely because Flexner's response had convinced him that he would have very little chance of finding a position in the United States. After mediation from the *Notgemeinschaft deutscher Wissenschaftler im Ausland* and the London-based *Joint Foreign Committee*, Reichenbach was offered both a temporary position at the University of Oxford and the chair of the philosophy department at the University of Istanbul, an institution that had recently been reorganized on a Western model by the secular Atatürk administration.²⁵ Reichenbach was greatly tempted by the Oxford offer but ended up signing a contract at the University of Istanbul (October 4, 1933, HRP, 041-11-01). The position in Turkey seemed more appealing because they offered a fixed position (July 28, 1933, HRP, 013-41-87) and because he would be joined by more than thirty other German academic refugees, including his friend and former colleague Erwin Finley Freundlich, who was offered the astronomy chair.

Reichenbach moved to Istanbul in October 1933. Initially, he was very happy with his new life in Turkey, despite the expected culture shock. In a letter to his former teacher Ernst von Aster, who would also be moving to Istanbul a few years later, Reichenbach wrote:

I have taken over the chair for general philosophy here, and I am quite happy with this solution ... I have been given the task of reorganizing the philosophical curriculum from the ground up, and this is a very interesting task; not easy, of course, especially since I still have to do everything with a translator. The students are very interested and, overall, very determined, which might help them overcome the difficulties they still encountered at the beginning. (Reichenbach to von Aster, January 7, 1934, HRP, 013-39-41)

Reichenbach quickly came to the conclusion that he had made a mistake, however. Already at the end of his first academic year, Reichenbach started to realize that the university would never be able to compete with institutions in the Western world. The students did not have the appropriate background, some of his Turkish colleagues were actively undermining his reform proposals, and the university administration was so bureaucratic that it took ages to get anything accomplished. In a letter to the Dutch physicist Jacob Clay, Reichenbach complained:

The university leaves a lot to be desired, unfortunately. There is a movement here within the administration that wants to suppress the university reform. In consequence, one always encounters unexpected resistance. The definitive orders for books and equipment that we have requested upon our arrival in October of last year, for example, still have not been placed ... In addition, teaching is also not very easy. The students here are not used to hard work, and one has to take into account their very basic level. I am particularly unlucky because my audience is exclusively made up of students who have no previous scientific training ... As a result, I can hardly speak about scientific matters. (July 6, 1934, HRP, 013-41-80)

Still half a year later, Reichenbach openly regretted that he had not accepted the Oxford offer, even though it had only been a one-year position. In a letter to his Oxford colleague Richard Crossman, Reichenbach explained the conditions at the University of Istanbul and described Oxford as a "paradise lost" (December 10, 1934, HRP, 013-41-83). It had been a mistake to try and introduce scientific philosophy in Turkey. The people were living in such "terrible poverty" (Reichenbach to Allen, August 24, 1934, HRP, 013-34-08) that the country would not be "needing scientific philosophy in the coming

²⁵ For a detailed reconstruction, see Irzik (2011).

50 years". The citizens of Istanbul required something more "elementary"—they needed "doctors, craftsmen, [and] middle schools" (Reichenbach to Landauer, August 6, 1936, HRP, 013-49-02).²⁶

As a result, Reichenbach reignited his plan to find a position in the United States. About a year after his move to Istanbul, Reichenbach wrote a letter to Allen, who had also helped him in his initial attempts to find a position across the Atlantic. Reichenbach explained that he did not want to stay in Istanbul and that he would be "very happy" if Allen could help him to "change Turkey for America" (October 25, 1934, HRP, 013-39-06).²⁷ Unfortunately, however, the increasingly desperate Reichenbach would have to wait four more years before he would be able to move to the United States. For although the American campaign for a position for Reichenbach would soon receive new impetus when Reichenbach met Charles Morris at the "Preliminary Conference of the First International Congresses for the Unity of Science" in September 1934, it would prove very difficult to find any opening in the United States—the country that, Reichenbach believed, was going to "continue German science" now that it had been "driven out of its home" (Reichenbach to Köhler, February 4, 1935, HRP, 013-48-25).

II.7. Logistic Empiricism

Reichenbach's renewed attempts to find a position in the United States were not only prompted by his dissatisfaction with the University of Istanbul. The Unity of Science conferences in the summers of 1934 (Prague) and 1935 (Paris) also played an important role in reigniting his interest in U.S. philosophy. Reichenbach met several American philosophers at both events and their contributions confirmed his suspicion that the "U.S.A. offers the best chances" for the development of scientific philosophy (Reichenbach to Hook, October 19, 1935, HRP, 013-46-72). Charles Morris presented papers about the strong thematic connections between American pragmatism and European empiricism (see section II.8), whereas Ernest Nagel and A. C. Benjamin illustrated Morris' point by reading papers about scientific reduction and operational definition.²⁸ Morris and Nagel convinced Reichenbach that his theories of induction and probability were very similar to those of Charles Sanders Peirce, a philosopher Reichenbach had never studied before (see section II.2). In the months after the Paris conference, Reichenbach read up on Peirce's work and confirmed Nagel's conclusion that he had

²⁶ In a letter to Lewin, who had chosen a temporary position at Cornell over a psychology chair at Istanbul, Reichenbach wrote: "I often have to think about how we sat in Berlin with your telegram from the United States and how we tried to decide between Turkey and America; I think you made the right decision back then ... the university [here is] terribly primitive, the students have zero background. The government, in addition, has no eye for science and ... is slow in financial matters... Moreover, there is a xenophobic nationalistic atmosphere here. It gives one the feeling of working for a lost cause" (December 13, 1934, HRP, 013-49-36).

²⁷ See also Reichenbach's letter to Einstein's son-in-law Rudolf Kayser, who had emigrated to the United States as well: "I was very happy to hear that you are so positive about America ... It has a magnetic effect on me. And it still tortures me very much that I have to sit here with my hands tied". In the remainder of the letter, Reichenbach asked Kayser to help him influence Flexner, as the latter would be "of course the first who could do something" (December 12, 1935, HRP, 013-48-09).

²⁸ See Benjamin (1936), Morris (1935ab), and Nagel (1935). See also Reichenbach to Kayser: "There was much interest in questions about induction and probability. This applies especially to the American participants, who told me that our philosophical theories are heavily discussed in America now" (October 26, 1935, HRP, 013-48-11). Reichenbach lists some of the American philosophers he met in his letter to Hook (October 19, 1935, HRP, 013-46-72).

independently developed an essentially Peircean view (Nagel 1934b, 592).²⁹ The United States, too, Reichenbach realized, had a rich tradition in scientific philosophy.

Reichenbach's conclusions about the development of U.S. philosophy of science were not merely positive, however. After the conference in Prague, Reichenbach became increasingly concerned that American philosophers equated Germanophone scientific philosophy with the views of the Vienna Circle, thereby ignoring the role his Berlin Group had played in its development. Although Morris had referred to Reichenbach in his talk about the similarities between American and European empiricisms, for example, he had classified him as a member of the Viennese school, describing the thematic unity between American and European philosophy as a unity between "the pragmatists ... and the Wiener Kreis" (Morris 1935a, 6). In his paper about scientific reduction, Ernest Nagel even ignored the views of Reichenbach and his group, referring exclusively to the connections between American philosophy and the views of the Vienna Circle (Nagel 1935, 46). Even though many U.S. philosophers were sympathetic to the development of philosophy of science in Europe, they appeared to be unaware of the contributions of Reichenbach's now scattered Berlin Circle.

Morris and Nagel were far from the only American philosophers to describe scientific philosophy as a Viennese invention. Although Blumberg and Feigl (1931) had introduced logical positivism as a "new movement in *European* philosophy", explicitly mentioning Reichenbach as one of its "foremost philosophical exponents" (p. 281, my emphasis),³⁰ American scholars were systematically referring to logical positivism as a *Viennese* research program, using labels like the "Vienna Circle", the "Viennese School", or the "Vienna positivists".³¹ Reichenbach had been the driving force behind *Erkenntnis*, the journal that had played an important role in disseminating European philosophy of science in the Anglophone world but philosophers were systematically referring to philosophy of science as a Viennese tradition, either by neglecting Reichenbach's contributions or by describing him as a member of the Wiener Kreis.

Even more problematic, from Reichenbach's perspective, was the American focus on Viennese theses like verificationism, phenomenalism, and *Aufbau*-style constitution theory; precisely those aspects of the *Wissenschaftliche Weltauffassung* the Berlin Group had been sceptical about from the very beginning. Whereas Reichenbach viewed scientific philosophy as a *method* or an *approach*, most American philosophers seemed to view it as a *philosophical system*. Indeed, most Anglophone publications about European philosophy of science focused on Carnap's constitution theory (e.g. Ginsburg 1932; Lewis 1934; Pratt 1934), the verifiability criterion of meaning (e.g. Black 1934; Dewey 1934; Nagel 1934a), or the Viennese thesis that metaphysics is meaningless (e.g. Gamertsfelder 1933; Weiss 1933; Morris 1934). The Berlin group's more liberal, probability-based, empiricism was largely ignored, despite Blumberg and Feigl's acknowledgment that there were "important differences between Reichenbach and the Viennese" on this score (1931, 291). This ignorance about the Berlin approach was especially frustrating to Reichenbach because many American commentators were, like him, arguing that the Viennese views were too radical. They simply failed to see that an (in Reichenbach's view) more subtle empiricism had already been developed.³²

²⁹ See Reichenbach's letter to Morris, in which he explains his surprise to find such "very modern views ... concerning probability and causality" in Peirce (December 12, 1935, HRP, 013-50-91).

³⁰ See section I.3.

³¹ See, for example, Ginsburg (1932), Gamertsfelder (1933), Weiss (1933), Lewis (1934), Pratt (1934), and Schilpp (1935). In fact, even Blumberg and Feigl themselves adopted the label "Vienna School" in subsequent work (e.g. Feigl 1934 and Blumberg 1935).

³² An additional source of frustration was that (former) members of the Vienna Circle seemed to reinforce the American misrepresentation of the development of scientific philosophy in Europe.

In an attempt to correct the American narrative about European philosophy of science, Reichenbach wrote a paper about the development scientific philosophy in Germany and submitted it to the American *Journal of Philosophy*. In the paper, titled “Logistic Empiricism in Germany and the Present State of its Problems”, Reichenbach aimed to do justice to the, in his view, correct history of the development of scientific philosophy in Europe. He introduced a new name for the movement (‘logistic empiricism’), distancing himself from the label “logical positivism” that had become so closely tied to the views of the Vienna Circle,³³ and he stressed its methodological nature, explicitly rejecting the doctrine-focused reception of scientific philosophy in the United States:

What unites ... this group is not the maintenance of a philosophical ‘system’, but a community of working methods—an agreement to treat philosophical problems as scientific problems whose answers are capable of soliciting universal assent. Philosophical problems, in other words, do not differ in principle from problems of the positive sciences. The strength of this group lies in its common working program and not in a common doctrine—a program which distinguishes it from philosophical sects, and makes possible progress in research. (1936a, 142)

Most importantly, however, Reichenbach used the paper to advertize the work of his former Berlin Group. He emphasized that his own *Relativitätstheorie und Erkenntnis apriori* (1920) had been the first publication to demand the “introduction of a method of analysis of science” (see section II.2); and he accentuated the Berlin Group’s focus on probability and induction, explaining their proposal to replace the standard two-valued logic with a continuous scale of probability values. The Berlin and Vienna circles were tightly connected through friendships, conferences, and *Erkenntnis*, Reichenbach explained, but the former group, unlike the Viennese, “concentrated on minute work”, insisting “that systematic construction must be foresworn until all details have been analyzed” (1936a, 150).³⁴

Reichenbach’s first American publication should not just be read as an attempt to correct the, in his view, one-sided U.S. reception of European philosophy of science, however. The paper was also an attempt to increase his chances on the American job market. For he submitted the paper in the summer of 1935, a few weeks after a major falling-out with the Turkish administration. In February 1935, Reichenbach had received a telegram from Hook, inviting him for a visiting professorship at

Reichenbach was particularly annoyed when he saw that Feigl, who had developed a theory of induction very similar to Reichenbach’s, had appended a note to his paper in the newly-founded journal *Philosophy of Science* explaining that he was gratified to see that “Reichenbach, after an Odyssey of attempts to found induction on probability” had “finally recognized our (Viennese) criticisms” and was “joining us now in the pragmatic view of inductive generalization” (Feigl 1934, 29). Reichenbach wrote an angry letter to Malisoff, the editor of the American journal, explaining that he had been the first to develop this position and that the Vienna Circle had always viewed the problem of the justification of induction as a pseudoproblem (October 6, 1935, HRP, 013-53-12). See Reichenbach (1936b).

³³ See Uebel (2013) for a detailed reconstruction of the history of labels like ‘logical empiricism’ and ‘logistic empiricism’. Uebel’s reconstruction suggests that Reichenbach invented the label especially for his *Journal of Philosophy* paper as he had seemed happy to use labels like ‘logistic neo-positivism’ (Reichenbach 1933, 201) and ‘logistical positivism’ (Reichenbach 1935a, 283) up to 1935.

³⁴ Reichenbach expresses a similar sentiment in a letter to von Aster, written in the same period. Focusing on the differences between Carnap and himself, Reichenbach writes: “I have ... consciously focused on solving certain singular problems because I saw that a comprehensive system would not be possible without solving them first... In Carnap’s work, you see all the dangers of a too early systematization. Again and again, he has been forced to withdraw his systems” (June 3, 1935, HRP, 013-39-34).

NYU for the upcoming academic year (HRP, 013-46-95)—a chance he had been waiting for ever since he had first asked Hook for help in the early 1930s (see section II.5). Because he was contractually obliged to stay in Istanbul until the summer of 1936,³⁵ Reichenbach had asked Hook whether a one-year postponement would be possible, explaining that the visiting professorship would be an ideal step toward a permanent position in the United States (March 11, 1935, HRP, 013-46-91). When postponement turned out to be impossible, however, Reichenbach and Hook had tried to arrange a leave of absence, or, if necessary, an annulment of his contract in Istanbul, so that Reichenbach could come to the United States “no matter what” (Reichenbach to Hook, March 31, 1935, HRP, 013-46-86). Hook had sent a lengthy cablegram to the Prime Minister (!) of Turkey, explaining why a one-year leave would be a beneficial arrangement for both NYU and the University of Istanbul, but the administration had refused, answering, without informing Reichenbach, that they would keep him to his contract and that they would only be willing to send a Turkish lecturer (April 22, 1935, HRP, 013-46-65). Reichenbach was deeply disappointed by the administration’s response and decided to resign in Istanbul as soon as he was legally permitted to do so, adding that it was now “morally impossible” for him to stay in Turkey. In a letter to Hook, Reichenbach explained:

Instead of their vanity being flattered by your invitation it was grieved by the fact that it was no Turkish man who was invited... I know now that this post in Turkey was a great mischief for me... My decision [has been] greatly increased by the collision I had with the rector in the matter concerning their way of deciding the question of my leave ... they answered you without informing or asking me... The consequence is that I want to make use of the first date for my giving notice here, and that I must look in any case for a new post in 1936. (April 13, 1935, HRP, 013-46-76)

Reichenbach was now willing to accept any American offer, including invitations for summer teaching, as long as it could help him to leave Istanbul. His paper about the development of ‘logistic empiricism’ in Germany, written in the weeks after his falling-out with the Turkish administration, was his first attempt to increase his visibility on the American job market.

II.8. The Chicago Campaign

Reichenbach’s job hunt received new impetus when he asked Charles Morris for help at the Unity of Science Conference in Paris, a few months after his falling-out with the Turkish administration. Morris, a 34-year-old Chicago professor, had initially been somewhat sceptical about the feasibility of logical positivism. About a year before he first met Reichenbach, he had published a paper about what he deemed to be Carnap’s implausibly strong variant of solipsism (Morris 1934).³⁶ Building on the philosophy of his former teacher G. H. Mead, Morris had argued that we should not reconstruct the intersubjective world from first-person experiences but that scientific knowledge presupposes the intersubjective world as there is an intrinsically “social factor in verification” (p. 557). Feigl, however,

³⁵ Reichenbach had signed a five-year, automatically renewable contract and was contractually obliged to stay in Istanbul for at least three academic years.

³⁶ See section 1.3.

had convinced Morris that logical positivism and pragmatism are compatible and suggested that he visit the key centers of scientific philosophy on his upcoming trip to Europe.³⁷

In Europe, Carnap invited Morris for the impending unity of science pre-conference in Prague, asking him to prepare a paper about the connections between pragmatism and logical positivism (April 8, 1934, RCP, 029-04-22). Morris happily accepted and started preparing by reading some of Carnap's recent work. When he read the first draft of Carnap's reply to Lewis,³⁸ Morris immediately recognized that the former's views had drastically shifted "in the direction of pragmatism" (Morris to Carnap, May 21, 1934, RCP, 029-04-19), confirming Feigl's view that pragmatism and logical positivism were complementary movements. A few months later, in Prague, Morris coined the term 'scientific empiricism' to describe the combined efforts of the pragmatists and the Wiener Kreis, defining the view as the merger of an "empirical habit of mind with an emphasis upon logical analysis and conceptual clarification" (Morris 1935a, 6).

In the year that followed, Morris quickly became one of the central organizational forces in the rapidly expanding Unity of Science movement. He approached the Rockefeller Foundation for funding (January 28, 1935, USM, Box 1, Folder 15), he joined the board of Neurath's 'International Encyclopedia of Unified Science' project (March 16, 1935, *ibid.*), and he arranged a publication contract for the encyclopedia at the University of Chicago Press (December 24, 1935, UCA, Box 345, Folder 8). Most importantly, however, Morris devised a plan to make Chicago the international center of "scientific empiricism". He had already arranged a temporary visiting position for Carnap after he learned that the latter would be coming to the United States in connection with the Harvard tercentenary celebrations (Morris to Carnap, August 30, 1935, RCP, 029-04-01)³⁹ but when Reichenbach asked him for help in the summer of 1935, Morris quickly started a campaign to bring *both* intellectual leaders to Chicago. A few weeks after the Paris conference, Morris could already write Reichenbach that he had convinced his colleagues to officially propose adding "both you and Carnap as additions to our faculty" (November 25, 1935, HRP, 013-50-92).

In order to understand how a 34-year-old professor was able to sway an entire department to back his proposal to hire two German philosophers of science, it is important to consider the state of Chicago's department of philosophy in the early 1930s. A few years before Morris proposed to hire Carnap and Reichenbach, the department had lost four full professors (E. A. Burt, G. H. Mead, Arthur E. Murphy, and J. H. Tufts), who had all resigned to protest the actions of Chicago's president Robert M. Hutchins, an educational philosopher who was trying to reform the university's curriculum along neo-Thomist lines and who had attempted to force a number of Aristotelian-Thomist philosophers (Mortimer Adler, Richard McKeon, and Scott Buchanan) on the department. Especially Adler, who fiercely opposed pragmatism because it focused too much on 'man-centered' instead of 'god-centered' thinking (Dzuback 1991, 95), was distrusted by the Chicago philosophers as rumors had spread that he was hired to institute radical changes in the department. After Hutchins had sent a humiliating survey to several leading philosophers in the country, asking them to evaluate the quality of the current Chicago faculty, the four had decided to resign and to take up positions elsewhere.⁴⁰

³⁷ Indeed, Morris first wrote Carnap a few weeks after he met Feigl, telling him about their meeting, his plan to visit "Berlin, Vienna, and Prague", and his view that "pragmatism ... and logical positivism are complementary movements" (November 12, 1933, HRC, 029-04-25).

³⁸ See section I.9.

³⁹ See section I.8.

⁴⁰ See the joint resignation letter of Burt, Mead, and Murphy, which can be found in the Hutchins Administration Records (UCA, Box 163, Folder 12).

It is sometimes suggested that Chicago's department of philosophy lost its pragmatist signature when Mead and Tufts left in 1931. This is a mistake. When the professors resigned, Hutchins was forced to apologize to the philosophers, to move Adler to the law department, and to hire Charles Morris, who had been the department's first choice for a number of years. And although it is true that Mead and Tufts had been the best-known pragmatists of the department, virtually all remaining faculty members still identified as pragmatists or as philosophers of science: E. S. Ames had been a member of the influential 'Chicago School of Pragmatism' and explicitly described himself as a Deweyan philosopher; Charner Perry's work "was strongly influenced by pragmatism and its place in American philosophy" (Dzuback 1991, 186); A. C. Benjamin was a scientific philosopher (see section II.6) who would publish one of the first American textbooks on philosophy of science (Benjamin 1937); and Charles Hartshorne was the main editor of the collected works of C. S. Peirce, the first six volumes of which appeared between 1931 and 1935.⁴¹

Considering the history and the composition of Chicago's philosophy department, it is not surprising that Morris was quickly able to collect support for his proposal to turn Chicago into a center of scientific empiricism. Not only could Morris' plan help to restore the glory of the once famous Chicago school, recently humiliated by Hutchins' survey, Reichenbach and Carnap could also be expected to strengthen the ties between the philosophers and Chicago's natural and social scientists. The pragmatist school had traditionally had strong ties with the natural and social science departments but these relations had started to wane after the four professors resigned.⁴² If anything, the fourfold resignation appears to have helped Morris to gain support for his plan. Not only did it 'free up' space to hire new leading scholars, 'logical positivism' was also a movement that mostly excited the *new* generation of American philosophers (e.g. Benjamin, Blumberg, Goodman, Hook, Leonard, Nagel, Parry, Quine, and Weiss). As such, it is questionable whether Morris would have been able to gather as much support if the old guard had still run the department. Finally, the unusually flat hierarchical structure after 1931 likely accelerated Morris' standing within the department. When Ames, the last member of the 'old' Chicago school, retired in the mid-1930s, the faculty selected Morris to replace him as head of department (Laing to Hutchins, January 7, 1935, UCA, Box 163, Folder 13).

Unfortunately, however, Hutchins did not accept Morris' plan. Although the entire department backed the proposal, Hutchins refused to turn Chicago into a center for scientific philosophy. Not only did he ignore the department's request to appoint Morris as its new head—leaving the position empty between 1935 and 1940—he also blocked the latter's proposal to bring both Reichenbach and Carnap to Chicago. Hutchins was careful not to start a new revolt by allowing the department to make Carnap an offer (especially after the latter had made a very good impression during his three-month visiting professorship) but he refused to hire *two* scientific philosophers, allegedly on the ground that Reichenbach's work was more physics than philosophy (Morris to Reichenbach, January 26, 1936, HRP, 013-50-89). In the end, even Morris' subsequent attempt to offer Reichenbach at least a one-year visiting professorship failed.

Morris had more 'bad luck' in trying to arrange a position for Reichenbach. A few weeks before Carnap received an offer from the University of Chicago, he had also been offered a professorship at

⁴¹ The last remaining professor, T. V. Smith, is more difficult to classify but also seems to have been sympathetic to Morris' plan. Archival evidence suggests that he was already campaigning for a position for Carnap at the University of Chicago when Morris was still in Europe (Morris to Carnap, December 3, 1934, RCP, 029-04-06).

⁴² After he returned from Europe, Morris tried to reconnect by starting an interdisciplinary "scientia group" in Chicago, bringing together about "25 professors drawn from all departments" for regular discussion meetings (Morris to Neurath, May 25, 1935 and January 26, 1936, USM, Box 1, Folder 15).

Princeton's Department of Philosophy, where he had presented a paper during his American lecture tour in the spring of 1936.⁴³ When Morris learned about the Princeton offer, he immediately started a campaign to stimulate them to hire Reichenbach instead (Morris to Reichenbach, March 31, 1936, HRP, 013-50-82). Morris and Carnap quickly discovered, however, that Princeton would never hire a 'Jewish professor'.⁴⁴ And although Morris continued his campaign by recommending Reichenbach to a large number of other American universities,⁴⁵ Reichenbach was forced to stay in Istanbul, breaking his resolution to terminate his contract as soon as he was legally allowed to do so. In a letter to Morris, Reichenbach somberly concluded:

It is a great pity ... I know you did all you could... What [depresses] me most is that it is antisemitism which excludes me now from the U.S.A.... This is now Hitler's success: instead of producing a general feeling of nauseousness, in civilized countries, against antisemitism, Hitler has succeeded in making antisemitism outside Germany even stronger than before. (July 9, 1936, HRP, 013-50-74)

II.9. *Experience and Prediction*

Reichenbach did not give up when he learned that he had to stay in Istanbul for the time being. On the contrary, he reinforced his attempts to find a position in the United States. In order to increase his chances on the American job market, Reichenbach started several new projects aimed at marketing his work on the other side of the Atlantic. He started publishing responses to American philosophers who had been engaging with his work on probability (e.g. Reichenbach 1938b on Nagel; Reichenbach 1938c on Everett Nelson), he started to write about the theories of the major pragmatists (e.g. Reichenbach 1939 on Dewey's philosophy of science), and, most importantly, he completed his first English-language monograph, *Experience and Prediction*, in which he systematically presented his 'logistic empiricism' to the American philosophical community.⁴⁶

In *Experience and Prediction*, Reichenbach breaks with his earlier attempts to sell logistic empiricism as an approach rather than as a system of doctrines. Convinced that comprehensive philosophical systems receive more attention than piecemeal analyses of scientific theories, Reichenbach presents a wide-ranging overview of his ideas about meaning, truth, knowledge, perception, existence, induction, and the question of the external world, arguing that most of these

⁴³ See section I.9.

⁴⁴ In a letter to Reichenbach, Carnap explained that Princeton university was "very antisemitic" and that the department did not contain "any non-Arians", partly as a response to the "almost exclusively Jewish Institute" of Advanced Study (June 12, 1936, HRP, 013-41-1). Reichenbach was not the only scientific philosopher to be affected by institutional antisemitism in the United States. Already in 1931, Feigl had reported about the "colossal" antisemitism in a letter to Schlick (April 5, 1931, MSP, 99/Fei-19).

⁴⁵ In the 1935-36 academic year alone, Morris also tried to get Reichenbach a position at the University of North Carolina, the University of Illinois, Northwestern University, the University of Wisconsin, and the University of Michigan (Reichenbach-Morris correspondence, HRP, 013-50-[73-94]).

⁴⁶ Reichenbach had already started working on the book manuscript after his falling out with the Turkish administration (Reichenbach to Morris, October 15, 1935, HRP, 013-50-94) but he sped up the process when Morris suggested that it would be very good for his job prospects if the book "could appear in English in 1936 or early in 1937" (Morris to Reichenbach, May 19, 1936, HRP, 013-50-75).

traditional philosophical problems can be solved using his ‘logistic theory of probability’.⁴⁷ Reichenbach’s aim to market his philosophy in the United States is also manifested in his efforts to directly engage with prominent American schools of thought. Whereas his first American publication (see section II.6) was mostly an attempt to modify the U.S. reception of the *European* development of scientific philosophy, mentioning the pragmatists only in passing,⁴⁸ *Experience and Prediction* explicitly includes “the American pragmatists and behaviorists” in the definition of ‘logistic empiricism’ in the opening paragraph of the book (1938a, v). In addition to the ‘positivists’, Reichenbach’s main conversation partners throughout the book are the pragmatists and the behaviorists. In a letter to Hempel, written after he just finished the first draft of his book, Reichenbach even describes *Experience and Prediction* as an anti-positivist and pro-behaviorist work:

I have been very busy with the manuscript for a new book, which is now finally finished ... It is a general epistemological work, quite anti-positivist and pro-behaviorist, and it is meant to show the implication of the concept of probability for very general questions, such as the problem of the external world, where I think the positivists have done a lot of harm. (December 27, 1936, HRP, 013-46-08)

A final novelty in *Experience and Prediction* is the central role of Reichenbach’s theory of meaning. Although Reichenbach had been strongly opposed to the idea that scientific philosophy is predominantly focused on questions about meaning,⁴⁹ *Experience and Prediction* puts Reichenbach’s theory of meaning center stage. Whereas positivists rely on a strictly verificationist theory, Reichenbach developed a “probability theory of meaning” consisting of the following two principles:

- (1) a proposition has meaning if it is possible to determine a weight, i.e., a degree of probability, for the proposition; and
- (2) two sentences have the same meaning if they obtain the same weight, or degree of probability, by every possible observation. (1938a, 54)

Reichenbach’s justification for the probability theory of meaning was mostly pragmatic: since large classes of sentences are not directly verifiable but can only be inferred with some probability from observation sentences, we have to choose between renouncing those sentences as meaningless and rejecting strict verifiability as a criterion of meaning. Naturally, Reichenbach picked the latter option: If we adopt the pragmatist view that “there is as much meaning in a proposition as can be utilized for action”, the probability theory of meaning is clearly superior, showing that there is “a close relation of the probability theory of meaning to pragmatism” (pp. 79-80).

⁴⁷ In the preface of his book, Reichenbach justifies this shift toward a comprehensive philosophical system by arguing that he had to develop a satisfactory theory of probability before he could turn to “an application of these ideas to questions of a more general epistemological character” (1938a, vi).

⁴⁸ “We ... invite empiricists and logisticians of all the world to share in our discussions. The first steps have already been taken through the establishment of relations between our group and Polish logisticians, French empiricists, American pragmatists, and some isolated philosophers scattered over the world” (Reichenbach 1936a, 160).

⁴⁹ After Reichenbach had read Nagel’s “Impressions and Appraisals of Analytic Philosophy in Europe (1936ab), for example, he had responded quite dismissive because Nagel had wrongfully suggested that scientific philosophers are mainly concerned with “abstract discussions about ‘meaning’” (Reichenbach to Nagel, February 15, 1936, HRP, 013-51-01).

In later chapters, Reichenbach also questions positivism by extending his probability theory of meaning to *direct* sentences, arguing that neither physicalistic observation sentences (p. 87) nor immediate sense experiences (p. 188) can be directly verified. Whereas most philosophers and psychologists have been swayed by the argument that we cannot doubt the existence of our impressions, Reichenbach follows the pragmatists and certain behaviorists—he explicitly mentions Dewey (1929) and Tolman (1935)—in resisting this “positivistic dogma” (p. 163). Not surprisingly, Reichenbach defends the view that impressions are *inferred* entities, arguing that we posit them to explain the difference between our dream world and the world we experience when we are awake. Propositions about impressions, too, have a probabilistic character, a conclusion that, Reichenbach argues, “has farreaching consequences” (p. 187):

This is the last blow against the positivistic theory, shaking even the last remnant of absolutism still left to it after the rejection of its wider pretensions ... There is no certainty at all remaining—all that we know can be maintained with probability only. There is no Archimedean point of absolute certainty left to which to attach our knowledge of the world; all we have is an elastic net of probability connections floating in open space. (1938a, 192)

Reichenbach, in sum, ends up with an anti-foundationalist theory that, in some respects, is closely aligned with the views of Peirce, Dewey, and Lewis. In fact, some of Reichenbach’s words seem to be chosen to echo the views of the pragmatists. Although Reichenbach does not explicitly draw the connection, his net metaphor and use of the phrase ‘Archimedean point’ appear to be nods to Lewis, who had used the same metaphors in *Mind and the World Order*, one of the books that had initially sparked Reichenbach’s interest in American anti-foundationalist philosophy about eight years earlier.⁵⁰

II.10. Coming to America

Experience and Prediction appeared in February 1938. Naturally, it was Morris who had arranged a publishing contract for Reichenbach with the University of Chicago Press. In the end, however, Reichenbach would not need the book to find a position in the United States. For, in the months before its publication, he received a cablegram from Robert Gordon Sproul offering him a “professor appointment” at the UCLA (November 14, 1937, HRP, 038-13-159). Reichenbach, who had been worried that he would have to prolong his contract with the University of Istanbul (Reichenbach to Morris, October 22, 1937, HRP, 013-50-50), was tremendously relieved when he received the offer, which had again been arranged by Morris. The Chicago philosopher had been exploring the option via his former colleague Donald Piatt, who had recently become the chair of the UCLA philosophy department. In about the same period, Morris also helped Reichenbach and Carnap move *Erkenntnis* to the United States when “influential groups” in Germany started to exert “strong pressure ... to discontinue the journal because of the number of Jewish collaborators” (Carnap to Conklin, December 12, 1937, USM, Box 1, Folder 4). In collaboration with the Dutch publisher Van Stockum & Zoon, Chicago University Press rebranded the venue as *The Journal of Unified Science*, a title Reichenbach grudgingly accepted as he had preferred the name “Journal of Logistic Empiricism” (Reichenbach to Morris, December 1, 1937, HRP, 013-50-47).

Now both Reichenbach (1936a, 1938a) and Carnap (1936, 1937) had abandoned the label ‘logical positivism’, many scientific philosophers started to follow suit, describing themselves as

⁵⁰ This is not to say though that Reichenbach fully agreed with Lewis’ theory. See footnote 15.

‘logical empiricists’ from the late 1930s onwards.⁵¹ And although this new, broadly shared label suggests a strong sense of unity, there have always been two competing stories about the differences between ‘logical positivism’ and ‘logical empiricism’. Some suggest that ‘logical empiricism’ grew out of Carnap’s (and Neurath’s) left wing Vienna Circle, culminating in Carnap’s ‘Testability and Meaning’,⁵² whereas others suggest that ‘logical empiricism’ explicitly refers to a tradition that had started in Berlin, where Reichenbach and his group developed the view as an alternative to the Viennese positivists.⁵³ In this two-part paper, however, I have argued that we cannot understand the meaning of either label without taking into account the attempts of the Vienna Circle and the Berlin Group to influence the American reception of scientific philosophy. The term ‘logical positivism’ was invented by Blumberg and Feigl to promote the views of the ‘new movement in European philosophy’ across the Atlantic, whereas ‘Testability and Meaning’, ‘Logistic Empiricism in Germany’, and *Experience and Prediction* were attempts to revise the American response to logical positivism: Carnap wanted to show that his views were not as strict as especially Lewis had made them out to be, whereas Reichenbach aimed to draw attention to the role his Berlin Group had played in the development of scientific philosophy.

Carnap’s and Reichenbach’s publications were not only attempts to influence the American reception of logical positivism, however. First and foremost, they wrote these works in order to increase their chances of finding positions in the United States. Both philosophers had developed an interest in moving to the U.S. in the early 1930s and became increasingly desperate to emigrate after the political developments of 1933. Despite their status among scientific philosophers as well as their rising fame in the United States, however, both Carnap and Reichenbach experienced great difficulties in finding a position in a country that was (1) crippled by the effects of the great depression, (2) at least in some places vehemently opposed to the positivists’ anti-metaphysical rhetoric, and (3) at least to some extent plagued by institutional antisemitism. It is only because of the organized and sustained efforts of a large number of American sympathizers (most notably Morris and Quine, but I have also discussed the contributions of i.a. Allen, Curtis, Feigl, Henderson, Hook, Lewis, Nagel, Perry, Weiss, and Whitehead) that both philosophers were ultimately able to find positions in the United States.

All in all, this two-part paper has aimed to show that it is a mistake to view Carnap’s and Reichenbach’s emigration as the starting point of scientific philosophy in the United States. There was (1) much American attention for logical positivism before Carnap and Reichenbach crossed the Atlantic and (2) European scientific philosophers had showed a keen interest in the work of both established (e.g. Bridgman, Dewey, Huntington, and Lewis) and young (e.g. Morris, Nagel, Parry, and Quine) American philosophers of science since the early 1930s. When Carnap and Reichenbach gave their first lectures on American soil, in other words, there was already a substantive community of scientific

⁵¹ The term ‘logical positivism’ remained widely popular *outside* the inner circle of scientific philosophers, likely fueled by the increasing number of text books and critical analyses that kept using the label (e.g. Ayer 1936; Weinberg 1936; Werkmeister 1937).

⁵² Indeed, Neurath had coined the term ‘logical empiricism’ in a 1931 article, four years before Reichenbach wrote ‘Logistic Empiricism in Germany’ (Neurath 1931/1983, 52). In addition, Carnap and Neurath had debated whether or not to adopt the term ‘logischen empirismus’ in the summer of 1935, probably in response to Morris’ 1934 proposal to adopt the term ‘scientific empiricism’ (Carnap-Neurath correspondence, July 1935, RCP, 029-09-[15-36]). See also Uebel (2013, section 5).

⁵³ See, e.g., Salmon (1999, 33): “To say that we live in a post-positivist age has been a cliché for decades, often uttered by those who have no understanding of the difference between the logical positivism of the Vienna Circle and logical empiricism, which originated in Berlin and completely superseded positivism in the second half of the twentieth century”.

philosophers in the United States—a community, moreover, that was actively pushing for a more modest variant of scientific philosophy. Although logical empiricism originated in Berlin, Prague, Vienna, and Istanbul, in sum, we can better understand its subsequent development if we take into account the substantive Euro-American interactions in the years before Carnap and Reichenbach left Europe.

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